

## Abandoning treatments that you have used for years is difficult

EDITOR—Why is it so difficult to put research findings into practice, especially when a traditional treatment is shown to be of little value? Christopher Del Mar and colleagues report their meta-analysis of antibiotic treatment for children with acute otitis media.<sup>1</sup> Three days later a general practitioner colleague brought her family to see me in an evening surgery. Her 10 month old daughter had had four infections in total; the first two she had diagnosed as acute otitis media and treated with erythromycin, the third was a minor gastroenteritis, and the last was thought to be another ear infection worthy of antibiotic treatment. By the fourth day of the illness, however, when the child was brought to my surgery, a rash had developed, giving the clue to the viral nature of the infection. The reason for the consultation was that the child's father, having just returned from the United States, where "putting tubes in is routine," was concerned that the child might need grommets to prevent further ear infections and deafness. I pointed out that the evidence for benefit from this operation was weak and heard myself saying to the parents, "I think you're doing the right thing by simply treating each infection as it arises." Then I remembered the paper I had just read, clearly showing that antibiotic treatment conferred no benefit in terms of the risk of further infections or long term deafness; only marginal benefit in terms of the control of symptoms; and a doubling of the chance of vomiting, diarrhoea, or rashes. The mother had also read the paper but, like me, had assumed that we would go on treating her child with antibiotics for any acute ear infection. Why? While I avidly take up new treatments with proved benefits, such as the eradication of *Helicobacter pylori* or anticoagulation in atrial fibrillation, when years of practice are overturned and shown to be of little value it is all but impossible to switch to doing nothing. My perception of what my patients have come to expect must play a major part.

If we could remove the obstruction to implementing research which shows that a treatment can be safely abandoned, then we could reduce unnecessary side effects and consultations and save money for more effective treatments as well. It seems a crucial step. A radical suggestion would be to exclude from the NHS any treatments proved to have no benefit; patients could still have them if they wished, but it would seem reasonable to ask them to foot the bill for expensive placebos.

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1 Del Mar D, Glasziou P, Hayem M. Are antibiotics indicated as initial treatment for children with acute otitis media? A meta-analysis. *BMJ* 1997;314:1526-9. (24 May.)

## Authors' reply

EDITOR—As the Cochrane version of our review of antibiotics and otitis media will be continuously improved and updated, we are

pleased to receive comment.<sup>1</sup> In answer to Helle Krogh Johansen and Peter C Gøtzche, we identified eight (not six) trials of antibiotics versus no antibiotics (as shown in our table 1). Only six of these studies reported clinically relevant outcomes. Rosenfeld et al's meta-analysis identified four trials of antibiotics versus no antibiotics and 29 comparing different antibiotics.<sup>2</sup> Because we identified twice as many studies for the principal question and our principal outcomes were patient centred (rather than microbiological), we believed that an update was required. One main outcome is pain, which by 24 hours is present in only 40% of patients. By two to seven days pain is present in only 14% of patients. Because antibiotics afford no benefit at 24 hours, we calculated the number needed to treat for pain at two to seven days. We apologise for the approximation involved in using the odds ratio to estimate the number needed to treat (equals 17). The latest version of the Review Manager software (REVMAN v3) allows two better ways of calculating the number needed to treat.<sup>3</sup> The relative risk of 64% means a 36% less chance of having pain at two to seven days if antibiotics were used initially. Since 14% will still have pain, the number needed to treat is  $1/(0.14 \times 0.36) = 20$ . Alternatively, directly estimating the risk difference in the meta-analysis gives a result of 0.038, with a corresponding number needed to treat of  $1/0.038 = 26$ . The authors correctly point out a transposition error in our figure 1—the subtotal for deafness at three months was carried over from the one month figure and should read 38/182 (treatment) and 49/188 (control); the summary odds ratios indicated in figure 1 are correct. The authors also point out that most of the information about vomiting, diarrhoea, or rashes comes from the study by Burke et al.<sup>3</sup> Unfortunately, only three of the eight trials report important side effects: clinical trials should report adverse as well as beneficial outcomes. Perhaps Jonathan E Osborne and Rhys T Nguyen are right to be concerned about severe infections and the developing world. We could not distinguish different outcomes between severe and milder cases. However, a policy of parsimonious antibiotic prescribing has not led to disaster in the Netherlands.<sup>4</sup> We sympathise with Ian Hill-Smith's dilemma, although we do not suggest that antibiotics are useless; their benefits are just rather modest. Accordingly, their use should be discretionary rather than either prohibited or mandatory.

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1 Glasziou PP, Hayem M, Del Mar CB. Treatments for acute otitis media in children: antibiotic versus placebo. In: Douglas R, Bernan S, Black RE, Bridges-Webb C, Campbell H, Glezen P, et al, eds. *Respiratory infections: module of the Cochrane database of systematic reviews*. Oxford: Update Software, 1996. [Database on disk and CD ROM.]  
2 Rosenfeld RM, Vertrees JE, Carr J, Cipolle RJ, Uden DL, Giebink G, et al. Clinical efficacy of antimicrobial drugs for acute otitis media: metaanalysis of 5400 children from thirty-three randomised trials. *J Pediatr* 1994;124:355-67.

3 Burke P, Bain J, Robinson D, Dunleavy J. Acute red ear in children: controlled trial of non-antibiotic treatment in general practice. *BMJ* 1991;303:558-62.  
4 Van Buchem FL, Peeters MF, van 't Hof MA. Acute otitis media: a new treatment strategy. *BMJ* 1985;290:1033-7.

## Relative risks are inflated in published literature

EDITOR—Relative risks are often reported incorrectly in medical journals. In a paper in the *BMJ*, Jian-Min Yuan and colleagues describe a relative risk of 3.72 for the relation between cancer of the upper aerodigestive tract and heavy drinking as a "3.7-fold increased risk"; this description is incorrect.<sup>1</sup> They also describe a relative risk of 1.30 for total mortality and heavy drinking as "a 30% excess risk"; this description is correct. They also write that "heavy drinking was associated with a significant 1.7-fold ... excess in risk of death from stroke"; the excess is 70%.

In an earlier paper by the same authors in *JAMA* there were similar problems.<sup>2</sup> A relative risk of 1.6 was correctly described as a "60% greater risk," while a relative risk of 2.3 was incorrectly described as a "2.3-fold excess risk." The paper also stated that "rates in Shanghai Chinese were 2-fold to 8-fold higher than in Los Angeles whites," but the rates in Shanghai were actually two to eight times those in Los Angeles whites. Such problems were not confined to the interpretation of relative risks. The sentence "In China, the yearly per capita consumption of cigarettes has increased 3-fold between the 1950s and 1987, from about 500 to 1748" is incorrect; the increase is actually twofold or 200%. Peto et al, in an accompanying editorial, stated "that heart attack mortality is five times lower, and that stroke mortality is five times higher."<sup>3</sup> This was a problem because the authors were referring to the ratio of 5.3 (366/69) and 1.42 (48/201) respectively.

I have found similar problems in reports by American, British, and Chinese authors (in alphabetical order). This problem is important when relative risks or differences of two measures are described. We have to be cautious about the confusing meaning of the suffix "-fold"; n-fold is equal to n times, and is equal to  $n \times 100\%$ . Therefore, a relative risk of 3.5 is 2.5-fold, or 2.5 times, or a 250% increase or excess in risk, not 3.5-fold or a 350% increase or excess. I wonder how long we will continue to see such inadvertent inflation of relative risks in the literature.

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1 Yuan J-M, Ross RK, Gao Y-T, Henderson BE, Yu MC. Follow up study of moderate alcohol intake and mortality among middle aged men in Shanghai. *China. BMJ* 1997;314:18-23. (4 January.)  
2 Yuan J-M, Ross RK, Wang X-L, Gao Y-T, Henderson BE, Yu MC. Morbidity and mortality in relation to cigarette smoking in Shanghai, China: a prospective male cohort study. *JAMA* 1996;275:1646-50.  
3 Peto R, Chen Z, Boreham J. Tobacco—the growing epidemic in China. *JAMA* 1996;275:1683-4.